The Temporary Thermo-Current in Iron.

In the *Philosophical Magazine* for January, Mr. Herbert Tomlinson has proposed an explanation of the remarkable fact that in an iron wire, heated red hot by a burner, an electric current is produced when the flame is shifted along the wire (see Wiedemann's "Galvanismus," ii. 453).

As his explanation is inadequate, perhaps I may be excused again drawing attention to this subject. Briefly his explanation is as follows:—That, as the portion of the wire in the flame rises in temperature, it, thermo-electrically speaking, becomes in fact like a different metal, and that then, on shifting the flame, the junction with the unaltered wire on the side moved towards becomes hotter than before, while the one on the other side falls in temperature, thus presenting the ordinary case of a thermo-couple with junctions at different temperatures. Now this explanation entirely overlooks the fact that, by the first assumption, just as fast as the temperature on one side rises, the wire there changes into the "second state," and correspondingly changes back on the other side as the temperature falls there; so that, as far as this explanation goes, there ought to be no current whatever, for thus both junctions must always be at the same temperature.

When I first noticed this current. which from considerations to follow I have ventured to call the "temporary thermo-current," it appeared to me to be due to the difference in the temperature-slope (or gradient) along the wire in front from that behind the flame, as it heats more rapidly in front than it cools behind, and to the electromotive force being a function of the slope, *i.e.* of $\frac{d\theta}{dx}$. But this hypothesis did not stand the test of experiment, as I have shown in a paper published in the Proceedings of the Royal Dublin Society, July 1886. So that as there *is* a current, we must suppose the "second state" to be not only a function of the temperature, but also of the time, *i.e.* that the wire changes into (or from) the "second state" more slowly than it is possible for it to change in temperature. So that the electromotive force at any point depends on the rate of change of the temperature slope, or equals $\phi\left(\frac{d}{dt}\frac{d\theta}{dx}\right)$. In support of this it will be found that if the flame be steadily moved along very slowly no current is produced—at all events less than would be otherwise expected; and, secondly, that the maximum current is got by moving the flame the fastest consistent with the condition of keeping the wire red hot.

It is with the view of emphasizing this dependence on the time that the term "temporary thermo-current" seems appropriate. Fred. T. Trouton.

Physical Laboratory, Trinity College, Dublin.

Causes influencing the Bathymetrical Range of Deep-Sea Fishes.

You refer (p. 219) to the fact that Dr. Günther has adopted the 100-fathom line as the boundary at which with the extinction of sunlight the bathybial fauna commences. This selection of 100 fathoms as the limiting horizon is of much interest in connection with the theory that the shallow-water marine fauna is greatly influenced by wave-currents. In a letter you published in 1885 (Nature, vol. xxxii. p. 390) I indicated 100 fathoms as the depth to which wave-action of some sort must extend, as evidenced by the character of the deposits at the mouth of the English Channel. Dr. Günther now shows that the deep-sea fishes do not rise above that horizon. But, although the 100-fathom horizon agrees very well with the apparent limit of wave-action, it does not seem to agree with the most recent experiments on the penetration of sunlight in water.

So recently as November last you recorded the fact that during the past year Prof. Forel found that the greatest "depth-limit of absolute darkness" from March to July in the Lake of Geneva was 100 metres (Nature, vol. xxxvii. p. 88). If experiments in a fresh-water lake may be taken as a guide to light-penetration in the ocean, 50 fathoms will be nearer the limit than 100. In this case the bathymetrical range of the bathybial fauna cannot be much influenced, if at all, by the presence or absence of sunlight. This view is moreover fortified by the fact that, though the deep-sea forms do not usually ascend above the 100-fathom line, the shallow-water forms go far below it; and there is no reason why they should not do so; for, although a form unfitted to withstand wave-currents cannot face them, there is nothing to prevent a flat-fish, fully equipped in this respect, from passing at will from the disturbed to the tranquil horizon, and *vice versâ.* A. R. Hunt.

Torquay, January 10.

Wind Force at Sea.

In reference to a letter on the above subject in Nature (p. 274), I beg to acquaint your readers that Capt. Barker's wish that anemometers should be used more on board ship has been endeavoured to be met by an instrument designed by myself on the sail principle. It has now been in use on some ships at sea for long voyages for five years, and daily observations have been obtained and sent home of the data observed, of pressure, direction, and velocity of the winds met with.

Regarding the further inquiry of ascertaining the rainfall at sea, this has now been carried on for about ten years by means of a rain-gauge designed by myself on the pivot principle, and it has been used by many vessels in all the great seas.

The daily observations have been sent home and are now on hand, and about five years of the returns have already been announced, and a further compilation of the data may be prepared when the materials become sufficient.

It may be added that the late Capt. Symington, of the s.s. *Hankow*, amongst his meteorological observations took the rainfall by rain-gauge on his ship for twenty years or more. The marine anemometer and rain-gauge above mentioned were exhibited at the Liverpool Exhibition in 1886, and at the Meteorological Exhibition of last year. W. G. Black.

Edinburgh, January 21.

Untimely Insect Development.

Some of your readers may be interested in a case of untimely insect development, caused no doubt by the phenomenal mildness of the weather in this part of the country during the last few days. Last evening a perfect imago of the common tortoise-shell butterfly (*Vanessa urtica*) was found inside my house on the wall of my nursery. It is fully developed in every way, and the only thing in its appearance at all abnormal is that the antennæ are bent back and lie between the wings, which are in the erect position usual in repose. The insect has evidently only just emerged from the pupa, and is in a torpid condition, only just flapping its wings when touched. The nursery is a warm room looking to the south, and has a fire in it all day.

St. Albans, January 10. John Morison.

Weasels killing Frogs.

Seeing a note in Nature (December 29, 1887, p. 208), about weasels killing frogs, I thought that the following fact would be a further confirmation.

I was walking near the village of Clifton Hampden in August last, when I saw a weasel, carrying a good-sized frog in its mouth, come cautiously out of the rank grass by the road-side; directly the weasel perceived me, it dropped its prey on the road and retreated to the cover of the grass. The frog was dead. I kept silence, and the weasel left its hiding-place, and advanced a few steps, but again retreated. Soon, after several advances and retreats, it rushed out, seized the frog with its teeth, and running across the road disappeared in the long grass on the other side.

January 20. M. S. Pembrey.

"British and Irish Salmonidæ."

The author of "British and Irish Salmonidæ" calls in question the justice of three criticisms in my review of that book. In reply to his first objection, I have to point out that my quotation of the sentence referred to was, as Mr. Day has himself noted, made to draw attention to its grammatical errors, and therefore the omission of a few words which affected the sense but not the construction was of no consequence at all. I omitted the words intentionally, to shorten the quotation, and gave no opinion on the statement contained in the sentence: the statement which is implied rather than expressed is perfectly correct.

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With regard to the second point, the statement in the text of the book which I questioned is as follows :—"The main principle is to employ thin layers of well-packed and pressed moss in trays with perforated bottoms, *the eggs being separated from the moss by muslin, mosquito-netting, swans' down, calico, or butter-cloth*, and that each tray contains two or three layers." In all the methods of packing salmonoid eggs in which moss is employed, the descriptions I have read state that the eggs are placed in direct contact with the moss, and Mr. Day does not justify the statement above quoted by referring to another statement in his notes, that for shorter journeys eggs are thrown off the frames on to swans' down. I doubted, and still doubt, if there is any method practised in which layers of moss are used, and are separated from the eggs by muslin or similar material.

With regard to the third point, it is true that on p. 249 of his book, in the chapter on *S. fontinalis*, Mr. Day refers to Brown Goode's "Game Fishes of the United States," and to the statement in that work that *S. namaycush* has, as its nearest relative, *S. fontinalis*. But I think a more direct reference to a speciegraphical description of *S. namaycush* might have been expected in a footnote referring to errors in the descriptions of this fish by certain writers. My remark about the omission of such reference was not made under the impression that S. *namaycush* was not a char, for I am aware that it is described as such in recent American reports on pisciculture, and have no doubt that such description is correct. But reference to speciegraphical determinations are rare in such reports, and I think readers of Mr. Day's book would have been glad of the references which he now supplies in his letter. YOUR REVIEWER.

MODERN VIEWS OF ELECTRICITY.[1]

PART III. (*continued*).

VI.

LET us now pass in review the various facts and experiences which have led us to a dual view of electricity; a kind of two-fluid theory, but in a very modified form.

First, there are the old experiments which vaguely suggest the separate existence of negative electricity, such as :—

(1) The wind from a point whether positive or negative; so that a candle gets blown always away from it, whether the point be on the prime conductor and the candle held in hand, or whether the point be held in the hand and presented to the candle or prime conductor; so, also, that a point whirligig turns the same way, whether supported on the prime conductor, or whether attached to the earth and placed near it.

(2) Phenomena connected with the spark discharge, such as Wheatstone's old experiment on what he called the velocity of electricity, with the three pair of knobs; and the double burr produced in cardboard when pierced with a spark, suggesting that something has pierced it both ways at once.

Then there are the more recently observed facts; as, for instance :—

(3) The fact that an electrostatic strain scarcely affects the volume of a dielectric; thereby at once suggesting something of the nature of a shearing or distorting stress, which alters shape but not size; a displacement of positive outwards and simultaneous negative inwards.

(4) The facts of electrolysis, and the double procession of atoms past each other in opposite directions.

(5) The phenomena of self-induction, and the behaviour of a thick wire to an alternating current. The delay also in magnetizing iron, and especially the possibility of permanent magnetism; combined with

(6) The absence of momentum in an electric current, or moment of momentum in an electro-magnet, as tested by all mechanical means yet tried.

I admit at once that many of these are mere superficial suggestions which may hardly bear examination and criticism. Only (3), (4), (5), and (6) can be at all seriously appealed to; but (5) and (6), in conjunction, seem to me to afford a sort of provisional and hypothetical proof, which (3) greatly strengthens.

At this point we must for the present again leave the question.

Representation of a Magnetic Field.

The disturbance called magnetism, which we have shown to be something of the nature of a spin—a rotation about an axis—is conspicuously not limited to the steel or iron of the magnet: it spreads out through all adjacent space, and constitutes what is called the magnetic field. A map of the field is afforded by the use of iron filings. which cling end to end and point out the direction of the force at every point.

These lines of force so mapped are to be regarded as the axes of molecular whirls. They are continuous with similar lines in the substance of the steel, and every line really forms a closed curve, of which a portion is in the steel and a portion in the air. In a wire helix, such as Figs. 16 or 29, the lines are wholly in the air, but in one part of their course they thread the helix, and in another part they spread out more or less between its faces.

But according to Ampère's theory of molecular currents there is no *essential* difference between such a helix and a steel magnet; directly the currents in the molecules of the magnet are considered, everything resolves itself into chains of molecular currents, threading themselves along a common closed curve or axis. Each atom, whether in the steel or in the air, is the seat of a whirl of electricity, more or less faced round so as on the average to have its plane at right angles to the lines of force. The simplest plan of avoiding having to consider those only partially faced round, is to imagine the whole number divided into a set which face accurately in the right direction, and a set which look any way at perfect random, and to neglect this latter set.

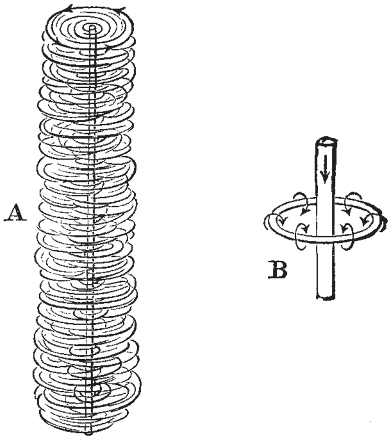

FIG. 30.—A, an element of a magnetic line of force with the electric whirl round it; B, a bit of an electric circuit with one of its magnetic lines of force shown round it, and the electric whirl round this; each magnetic line of force round a current being an electric vortex ring.

Well now try and picture a chain of whirls like beads spinning on a wire threading them all, and think of the effect of a material fluid thus rotating. Obviously it would tend to whirl itself fatter, and to shorten its length. An assemblage of such parallel straight whirls would thus squeeze each other laterally, or cause a lateral pressure, and would tend to drag their free ends together, causing a longitudinal tension.

Such whirls cannot in truth have free ends except at the boundary of a medium—as at the free surface of a liquid. Magnetic whirls are in reality all closed curves;

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[1] Continued from p. 110.

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